a system. Why should they? Does the desire to beat competitors stimulate a desire for knowledge? Does it stimulate originality? I for one would willingly see them non-existent.

Up to a certain point, the acquisition of knowledge of facts should be, as at present, tested by examination; but I am convinced that the system is at present pushed to an extreme, and that much better results would be gained by giving a degree for training, and that can be done only by the trainer—the teacher. He will, as a rule, be glad to share his responsibility with, and to benefit by the advice of, an outsider; but with him should ultimately rest the decision as to the merit or demerit of a candidate, as he is the only person able to judge. Under such a system, there would be little plucking; for the student would be advised not to present himself, unless he had sufficiently qualified.

It may also be said that undue advantage would be taken by the teacher in recommending unfit students for graduation. Teachers in such positions are, I believe, generally honourable men; they are chosen after the most careful inquiry into their past career. It is not held fitting in commercial circles to appoint a clerk or an accountant on good recommendations, and after sufficient apprenticeship, and then to surround him with safeguards, in case he turn out incapable or dishonest.

The objection may possibly be raised, that under such a system the standard of degrees would be very uneven; but what of that? As at present, anyone applying for a post of any kind would furnish a reference to his teachers; and a private letter from one well acquainted with the candidate turns the scale, for or against, in spite of every degree in the United Kingdom.

In plain English, degrees, as at present given, are not valued by that portion of the public qualified to judge; and we must face this fact, and endeavour to render a degree a real mark of merit.

I believe, with Mr. Dickins, that the examinations of the University of London have done much in disseminating knowledge, and they have therefore proved of great service, but except in the case of the higher degrees before mentioned, and of the degrees in the Faculty of Medicine where evidence of training is a *sine quâ non*, I greatly doubt whether they have contributed towards the creation of knowledge, or training in originality. And from the very nature of the constitution of the University of London, it is impossible that it should be otherwise. This very morning, I happened to ask a student attending my lectures on organic chemistry why he, a B.Sc. in chemistry, was attending my lectures. His reply was characteristic. "I scamped up enough of the subject privately, sir, to squeeze through; but now I wish to know it." In any right system, such a proceeding should be impossible.

It is therefore with the hope that the creation of a teaching University for London might tend to remedy such evils, that I, for one, would welcome it. I would urge that the distinguished names mentioned by Mr. Thiselton Dyer are surely guarantees that the London Colleges recently possessed men capable of imparting the highest standard of knowledge, and of stimulating true originality; yet I believe that it is by no means "cutting cheese with a razor" to employ just such men in watching over the development even of junior students; and it is not without advantage to the most able men of science and of letters to be obliged periodically to devote consideration to "elements" and to pass in review first principles. It counteracts the tendency towards specialization, which, however valuable, always limits the mental horizon. I will undertake to say that the quality of the most advanced teaching in biology and physiology in University College when the chairs were occupied by Burdon Sanderson, by Michael Foster, and by Lankester knew no limit; and I greatly doubt the wisdom of appointing teachers whose attention is to be devoted exclusively to research. As my predecessor, Prof. Williamson, often remarked, it is more difficult to teach junior than to teach senior students; and while the superintendence of exercise and laboratory work may well be shared by assistants, in order that the professor may have time to devote to research, and to superintendence of advanced students, it would be a serious calamity were the influence of such minds to be withdrawn wholly from the juniors.

It is precisely by such a federation of Colleges such as University and King's, and of other sufficiently qualified institutions which have the will and the power to join, that specialization may ultimately be effected. The future occupants of the chairs may be chosen so as to represent every side of a subject; and anyone wishing to pursue research in any special branch would have no difficulty in selecting that particular college where his specialty was also the specialty of the teacher.

William Ramsay.

No well-wisher of the University can feel otherwise than grateful to you for affording a portion of your valuable space for the letters of Mr. Thiselton Dyer and Mr. Dickins on this subject. No two men could be found to speak with greater authority from first-hand knowledge of the facts. The arguments on the subject have been too much of an *ex parte* character hitherto, not seldom based on insufficient information or erroneous impressions. Nothing, for example, could be further from the truth than the statement in the *Times* of May 13, by the writer of what was upon the whole a fair and comprehensive leading article, that "there is no reason why the highest honours of the University of London should not be obtained by a person who never set foot in London or even in England." Many, who like myself voted for the projected scheme of the Senate, must have felt, as I did, as a result of a wide and varied educational experience, that it was potential with great good in the future, and could be accepted as the working basis of the future development of the University, although we felt that the one serious blot in it was the abandonment of uniformity in the examinations for the pass degrees. I verily believe that this was the one thing fatal to its success in Convocation; that it was so far in excess of the recommendations of the Royal Commission as to be unwarrantable; and that it put a lever into the hands of the opposition, of which—as the event proved—a practised disputant like Mr. Bompas did not fail to make most effective and disastrous use.

Wellington College, Berks, May 25. A. Irving.

Quaternions and the "Ausdehnungslehre."

The year 1844 is memorable in the annals of mathematics on account of the first appearance on the printed page of Hamilton's "Quaternions" and Grassmann's "Ausdehnungslehre." The former appeared in the July, October, and supplementary numbers of the *Philosophical Magazine*, after a previous communication to the Royal Irish Academy, November 13, 1843. This communication was indeed announced to the Council of the Academy four weeks earlier, on the very day of Hamilton's discovery of quaternions, as we learn from one of his letters. The author of the "Ausdehnungslehre," although not unconscious of the value of his ideas, seems to have been in no haste to place himself on record, and published nothing until he was able to give the world the most characteristic and fundamental part of his system with considerable development in a treatise of more than 300 pages, which appeared in August 1844.

The doctrine of quaternions has won a conspicuous place among the various branches of mathematics, but the nature and scope of the "Ausdehnungslehre," and its relation to quaternions, seem to be still the subject of serious misapprehension in quarters where we naturally look for accurate information. Historical justice, and the interests of mathematical science, seem to require that the allusions to the "Ausdehnungslehre" in the article on "Quaternions," in the last edition of the "Encyclopædia Britannica," and in the third edition of Prof. Tait's "Treatise on Quaternions," should not be allowed to pass without protest.

It is principally as systems of geometrical algebra that quaternions and the "Ausdehnungslehre" come into comparison. To appreciate the relations of the two systems, I do not see how we can proceed better than if we ask first what they have in common, then what either system possesses which is peculiar to itself. The relative extent and importance of the three fields, that which is common to the two systems, and those which are peculiar to each, will determine the relative rank of the geometrical algebras. Questions of priority can only relate to the field common to both, and will be much simplified by having the limits of that field clearly drawn.

Geometrical addition in three dimensions is common to the two systems, and seems to have been discovered independently both by Hamilton and Grassmann, as well as by several other persons about the same time. It is not probable that any especial claim for priority with respect to this principle will be urged for either of the two with which we are now concerned.

The functions of two vectors which are represented in quaternions by $S\alpha\beta$ and $V\alpha\beta$ are common to both systems as published in 1844, but the quaternion is peculiar to Hamilton's. The linear vector function is common to both systems as ultimately developed, although mentioned only by Grassmann as early as 1844.

To those already acquainted with quaternions, the first question will naturally be: To what extent are the geometrical methods which are usually called quaternionic peculiar to Hamilton, and to what extent are they common to Grassmann? This is a question which anyone can easily decide for himself. It is only necessary to run one's eye over the equations used by quaternionic writers in the discussion of geometrical or physical subjects, and see how far they necessarily involve the idea of the quaternion, and how far they would be intelligible to one understanding the functions $S\alpha\beta$ and $V\alpha\beta$, but having no conception of the quaternion $\alpha\beta$, or at least could be made so by trifling changes of notation, as by writing S or V in places where they would not affect the value of the expressions. For such a test the examples and illustrations in treatises on quaternions would be manifestly inappropriate, so far as they are chosen to illustrate quaternionic principles, since the object may influence the form of presentation. But we may use any discussion of geometrical or physical subjects, where the writer is free to choose the form most suitable to the subject. I myself have used the chapters and sections in Prof. Tait's "Quaternions" on the following subjects: Geometry of the straight line and plane, the sphere and cyclic cone, surfaces of the second degree, geometry of curves and surfaces, kinematics, statics and kinetics of a rigid system, special kinetic problems, geometrical and physical optics, electrodynamics, general expressions for the action between linear elements, application of ∇ to certain physical analogies, pp. 160–371, except the examples (not worked out) at the close of the chapters.

Such an examination will show that for the most part the methods of representing spatial relations used by quaternionic writers are common to the systems of Hamilton and Grassmann. To an extent comparatively limited, cases will be found in which the quaternionic idea forms an essential element in the signification of the equations.

The question will then arise with respect to the comparatively limited field which is the peculiar property of Hamilton, How important are the advantages to be gained by the use of the quaternion? This question, unlike the preceding, is one into which a personal equation will necessarily enter. Everyone will naturally prefer the methods with which he is most familiar; but I think that it may be safely affirmed that in the majority of cases in this field the advantage derived from the use of the quaternion is either doubtful or very trifling. There remains a residuum of cases in which a substantial advantage is gained by the use of the quaternionic method. Such cases, however, so far as my own observation and experience extend, are very exceptional. If a more extended and careful inquiry should show that they are ten times as numerous as I have found them, they would still be exceptional.

We have now to inquire what we find in the "Ausdehnungslehre" in the way of a geometrical algebra, that is wanting in quaternions. In addition to an algebra of vectors, the "Ausdehnungslehre" affords a system of geometrical algebra in which the point is the fundamental element, and which for convenience I shall call Grassmann's algebra of points. In this algebra we have first the addition of points, or quantities located at points, which may be explained as follows. The equation

$$aA + bB + cC + \&c. = eE + fF + \&c.,$$

in which the capitals denote points, and the small letters scalars (or ordinary algebraic quantities), signifies that

$$a + b + c + \&c. = e + f + \&c.,$$

and also that the centre of gravity of the weights a, b, c, &c., at the points A, B, C, &c., is the same as that of the weights e, f, &c., at the points E, F, &c. (It will be understood that negative weights are allowed as well as positive.) The equation is thus equivalent to four equations of ordinary algebra. In this Grassmann was anticipated by Möbius ("Barycentrischer Calcul," 1827).

We have next the addition of finite straight lines, or quantities located in straight lines (*Liniengrössen*). The meaning of the equation

$$AB + CD + \&c. = EF + GH + \&c.$$

will perhaps be understood most readily, if we suppose that each member represents a system of forces acting on a rigid body. The equation then signifies that the two systems are equivalent. An equation of this form is therefore equivalent to six ordinary equations. It will be observed that the *Liniengrössen* AB and CD are not simply vectors; they have not merely length and direction, but they are also located each in a given line, although their position within those lines is immaterial. In Clifford's terminology, AB is a *rotor*, AB + CD a *motor*. In the language of Prof. Ball's "Theory of Screws," AB + CD represents either a *twist* or a *wrench*.

We have next the addition of plane surfaces (*Plangrössen*). The equation

$$ABC + DEF + GHI = JKL$$

signifies that the plane JKL passes through the point common to the planes ABC, DEF, and GHI, and that the projection by parallel lines of the triangle JKL on any plane is equal to the sum of the projections of ABC, DEF, and GHI on the same plane, the areas being taken positively or negatively according to the cyclic order of the projected points. This makes the equation equivalent to four ordinary equations.

Finally, we have the addition of volumes, as in the equation

$$ABCD + EFGH = IJKL,$$

where there is nothing peculiar, except that each term represents the six-fold volume of the tetrahedron, and is to be taken positively or negatively according to the relative position of the points.

We have also multiplications as follows:—The line (*Liniengrösse*) AB is regarded as the product of the points A and B. The *Plangrösse* ABC, which represents the double area of the triangle, is regarded as the product of the three points A, B, and C, or as the product of the line AB and the point C, or of BC and A, or indeed of BA and C. The volume ABCD, which represents six times the tetrahedron, is regarded as the product of the points A, B, C, and D, or as the product of the point A and the *Plangrösse* BCD, or as the product of the lines AB and BC, &c., &c.

This does not exhaust the wealth of multiplicative relations which Grassmann has found in the very elements of geometry. The following products are called *regressive*, as distinguished from the *progressive*, which have been described. The product of the *Plangrössen* ABC and DEF is a part of the line in which the planes ABC and DEF intersect, which is equal in numerical value to the product of the double areas of the triangles ABC and DEF multiplied by the sine of the angle made by the planes. The product of the *Liniengrösse* AB and the *Plangrösse* CDE is the point of intersection of the line and the plane with a numerical coefficient representing the product of the length of the line and the double area of the triangle multiplied by the sine of the angle made by the line and the plane. The product of three *Plangrössen* is consequently the point common to the three planes with a certain numerical coefficient. In plane geometry we have a regressive product of two *Liniengrösse*, which gives the point of intersection of the lines with a certain numerical coefficient.

The fundamental operations relating to the point, line, and plane are thus translated into analysis by multiplications. The immense flexibility and power of such an analysis will be appreciated by anyone who considers what generalized multiplication in connection with additive relations has done in other fields, as in quaternions, or in the theory of matrices, or in the algebra of logic. For a single example, if we multiply the equation

$$AB + CD + \&c. = EF + GH + \&c.$$

by PQ (P and Q being any two points), we have

$$ABPQ + CDPQ + \&c. = EFPQ + GHPQ + \&c.,$$

which will be recognized as expressing an important theorem of statics.

The field in which Grassmann's algebra of points, as distinguished from his algebra of vectors, finds its especial application and utility, is nearly coincident with that in which, when we use the methods of ordinary algebra, tetrahedral or anharmonic co-ordinates are more appropriate than rectilinear. In fact, Grassmann's algebra of points may be regarded as the application of the methods of multiple algebra to the notions connected with tetrahedral co-ordinates, just as his or Hamilton's algebra of vectors may be regarded as the application of the methods of multiple algebra to the notions connected with rectilinear co-ordinates. These methods, however, enrich the field to which they are applied with new notions. Thus the notion of the co-ordinates of a line in space, subsequently introduced by Plücker, was first given in the "Ausdehnungslehre" of 1844. It should also be observed that the utility of a multiple algebra, when it takes the place of an ordinary algebra of four co-ordinates, is very much greater than when it takes the place of three co-ordinates, for the same reason that a multiple algebra taking the place of three co-ordinates is very much more useful than one taking the place of two. Grassmann's algebra of points will always command the admiration of geometers and analysts, and furnishes an instrument of marvellous power to the former, and in its general form, as applicable to space of any number of dimensions, to the latter. To the physicist an algebra of points is by no means so indispensable an instrument as an algebra of vectors.

Grassmann's algebra of vectors, which we have described as coincident with a part of Hamilton's system, is not really anything separate from his algebra of points, but constitutes a part of it, the vector arising when one point is subtracted from another. Yet it constitutes a whole, complete in itself, and we may separate it from the larger system to facilitate comparison with the methods of Hamilton.

We have, then, as geometrical algebras published in 1844, an algebra of vectors common to Hamilton and Grassmann, augmented on Hamilton's side by the quaternion, and on Grassmann's by his algebra of points. This statement should be made with the reservation that the *addition* both of vectors and of points had been given by earlier writers.

In both systems as finally developed we have the linear vector function, the theory of which is identical with that of strains and rotations. In Hamilton's system we have also the linear quaternion function, and in Grassmann's the linear function applied to the quantities of his algebra of points. This application given those transformations in which projective properties are preserved, the doctrine of reciprocal figures or principle of duality, &c. (Grassmann's theory of the linear function is, indeed, broader than this, being co-extensive with the theory of matrices; but we are here considering only the geometrical side of the theory.)

In his earliest writings on quaternions, Hamilton does not discuss the linear function. In his "Lectures on Quaternions" (1853), he treats of the inversion of the linear vector function, as also of the linear quaternion function, and shows how to find the latent roots of the vector function, with the corresponding axes for the case of real and unequal roots. He also gives a remarkable equation, *the symbolic cubic*, which the functional symbol must satisfy. This equation is a particular case of that which is given in Prof. Cayley's classical "Memoir on the Theory of Matrices" (1858), and which is called by Prof. Sylvester the Hamilton-Cayley equation. In his "Elements of Quaternions" (1866), Hamilton extends the symbolic equation to the quaternion function.

In Grassmann, although the linear function is mentioned in the first "Ausdehnungslehre," we do not find so full a discussion of the subject until the second "Ausdehnungslehre" (1862), where he discusses the latent roots and axes, or what corresponds to axes in the general theory, the whole discussion relating to matrices of any order. The more difficult cases are included, as that of a strain in which all the roots are real, but there is only one axis or unchanged direction. On the formal side he shows how a linear function may be represented by a quotient or sum of quotients, and by a sum of products, *Lückenausdruck*.

More important, perhaps, than the question when this or that theorem was first published is the question where we first find those notions and notations which give the key to the algebra of linear functions, or the algebra of matrices, as it is now generally called. In vol. xxxi. p. 35, of this journal, Prof. Sylvester speaks of Cayley's "ever-memorable" "Memoir on Matrices" as constituting "a second birth of Algebra, its *avatar* in a new and glorified form," and refers to a passage in his "Lectures on Universal Algebra" from which, I think, we are justified in inferring that this characterization of the memoir is largely due to the fact that it is there shown how matrices may be treated as extensive quantities, capable of addition as well as of multiplication. This idea, however, is older than the memoir of 1858. The *Lückenausdruck*, by which the matrix is expressed as a sum of a kind of products (*lückenhaltig*, or open), is described in a note at the end of the first "Ausdehnungslehre." There we have the matrix given not only as a sum, but as a sum of products, introducing a multiplicative relation entirely different from the ordinary multiplication of matrices, and hardly less fruitful, but not lying nearly so near the surface as the relations to which Prof. Sylvester refers. The key to the theory of matrices is certainly given in the first "Ausdehnungslehre," and if we call the birth of matricular analysis the second birth of algebra, we can give no later date to this event than the memorable year of 1844.

The immediate occasion of this communication is the following passage in the preface to the third edition of Prof. Tait's "Quaternions":—

"Hamilton not only published his theory complete, the year before the first (and extremely imperfect) sketch of the 'Ausdehnungslehre' appeared; but had given ten years before, in his protracted study of Sets, the very processes of external and internal multiplication (corresponding to the Vector and Scalar parts of a product of two vectors) which have been put forward as specially the property of Grassmann."

For additional information we are referred to art. "Quaternions," "Encyc. Brit.," where we read respecting the first "Ausdehnungslehre":—

"In particular two species of multiplication ('inner' and 'outer') of directed lines in one plane were given. The results of these two kinds of multiplication correspond respectively to the numerical and the directed parts of Hamilton's quaternion product. But Grassmann distinctly states in his preface that he had not had leisure to extend his method to angles in space. . . . . But his claims, however great they may be, can in no way conflict with those of Hamilton, whose mode of multiplying *couples* (in which the 'inner' and 'outer' multiplication are essentially involved) was produced in 1833, and whose quaternion system was completed and published before Grassmann had elaborated for press even the rudimentary portions of his own system, in which the veritable difficulty of the whole subject, the application to angles in space, had not even been attacked."

I shall leave the reader to judge of the accuracy of the general terms used in these passages in comparing the first "Ausdehnungslehre" with Hamilton's system as published in 1843 or 1844. The specific statements respecting Hamilton and Grassmann require an answer.

It must be Hamilton's "Theory of Conjugate Functions or Algebraic Couples" (read to the Royal Irish Academy 1833 and 1835, and published in vol. xvii. of the Transactions), to which reference is made in the statements concerning his "protracted study of Sets" and "mode of multiplying *couples*." But I cannot find anything like Grassmann's external or internal multiplication in this memoir, which is concerned, as the title pretty clearly indicates, with the theory of the complex quantities of ordinary algebra.

It is difficult to understand the statements respecting the "Ausdehnungslehre," which seem to imply that Grassmann's two kinds of multiplication were subject to some kind of limitation to a plane. The external product is not limited in the first "Ausdehnungslehre" even to three dimensions. The internal, which is a comparatively simple matter, is mentioned in the first "Ausdehnungslehre" only in the preface, where it is defined, and placed beside the external product as relating to directed lines. There is not the least suggestion of any difference in the products in respect to the generality of their application to vectors.

The misunderstanding seems to have arisen from the following sentence in Grassmann's preface: "And in general, in the consideration of angles in space, difficulties present themselves, for the complete (*allseitig*) solution of which I have not yet had sufficient leisure." It is not surprising that Grassmann should have required more time for the development of some parts of his system, when we consider that Hamilton, on his discovery of quaternions, estimated the time which he should wish to devote to them at ten or fifteen years (see his letter to Prof. Tait in the *North British Review* for September 1866), and actually took several years to prepare for the press as many pages as Grassmann had printed in 1844. But any speculation as to the questions which Grassmann may have had principally in mind in the sentence quoted, and the particular nature of the difficulties which he found in them, however interesting from other points of view, seems a very precarious foundation for a comparison of the systems of Hamilton and Grassmann as published in the years 1843-44. Such a comparison should be based on the positive evidence of doctrines and methods actually published.

Such a comparison I have endeavoured to make, or rather to indicate the basis on which it may be made, so far as systems of geometrical algebra are concerned. As a contribution to analysis in general, I suppose that there is no question that Grassmann's system is of indefinitely greater extension, having no limitation to any particular number of dimensions.

J. WILLARD GIBBS.

The Flying to Pieces of a Whirling Ring.

IN NATURE of May 14 (p. 31) I notice a letter by Mr. C. A. Carus-Wilson on the rotation of a hollow steel flask, composed apparently of a spherical shell mounted on an axis constituting a diameter. Mr. Carus-Wilson speaks of this body as being under a "tension" of "31·5 tons per square inch" at a certain speed of rotation. He does not, however, specify what is the tension to which he refers, nor where it is found, neither does he give the density and elastic constants of the material nor indicate the method by which he arrived at his result.

So far as I know, the only problem of the kind which has yet been solved is that of an isotropic spherical shell[1] rotating about an imaginary axis through its centre at speeds at which the strains follow Hooke's law. This differs from the case Mr. Carus-Wilson speaks of, inasmuch as the existence of a real material axis must introduce conditions somewhat different from those assumed by the mathematical theory, and further the results obtained by this theory cannot legitimately be applied to speeds exceeding that where bulging becomes sensible, if indeed so far.

This solution is probably, however, the nearest to the practical problem at present attainable.

According to it the strains and stresses vary throughout the shell with the distance from the centre, and the angular distance from the axis of rotation. They also depend on the density and on the elastic properties of the material. There are also at every point three principal stresses, whereof one it is true vanishes over the surfaces. Thus such a statement as Mr. Carus-Wilson's requires further explanation.

According to the two theories most commonly entertained, the quantity which determines the limiting safe speed is the maximum value of either the *greatest strain* or the *maximum stress-difference*,—*i.e.* the algebraical difference between the greatest and least principal stresses at a point. Over the surfaces of the shell the absolutely greatest values of both these quantities are found, for shells of all degrees of thickness, in the equatorial plane—or plane through the centre perpendicular to the axis of rotation.

Denoting the angular velocity by ω, the radii of the outer and inner surfaces respectively by a and a', the density by ρ, Young's modulus by E, the greatest strain by s, the maximum stress-difference by S, and the stress at right angles to the meridian plane by Φ, the three last quantities being measured in the equator, the following are some of the results I found for materials in which Poisson's ratio is 1/4 :—

	$Es/\omega^2\rho a^2$		$S/\omega^2\rho a^2$		$\Phi/\omega^2\rho a^2$	
	Inner surface.	Outer surface.	Inner surface.	Outer surface.	Inner surface.	Outer surface.
$a'/a = 0·9$	0·950	0·833	1·064	0·866	0·912	0·866
$\frac{a-a'}{a}$ negligible	1·0	1·0	1·0	1·0	1·0	1·0

Apparently in the case mentioned by Mr. Carus-Wilson, $a'/a = 15/16 = 0·9375$. Supposing the material to have Poisson's ratio $= 1/4$, which seems to accord fairly with experiments on steel, the approximate values of s, S, and Φ, for this case could be obtained by interpolation from those I give above. The differences between the values of corresponding strains and stresses at the two surfaces are less, of course, for $a'/a = 15/16$ than for $a'/a = 0·9$, but still are far from negligible. Mr. Carus-Wilson's numerical result rather suggests that the tension he refers to is the stress Φ, measured as above in the equator, and that he employed the formula $\Phi = \omega^2\rho a^2$. This formula (see Cambridge Philosophical Transactions, vol. xiv. p. 300), is correct for the value of Φ in the *equator* in an *infinitely thin* shell, but it does not strictly apply to any shell whose thickness is comparable with its radius. In the paper in the Cambridge Transactions first referred to, there are given tables of the numerical measures of the strains and stresses over the surfaces for a series of values of a'/a for materials in which Poisson's ratio is 1/4. These give by interpolation fairly accurate values for all values of a'/a. For other values of Poisson's ratio, recourse must be had to the general formulæ given in the paper, unless $\epsilon, \equiv 1 - a'/a$, is very small, when the greatest values of s and S are given approximately by $Es/\omega^2\rho a^2 = 1 - \frac{1}{2}\epsilon(1 - \eta)$, $S/\omega^2\rho a^2 = 1 + \epsilon/(1 + \eta)$, where η is Poisson's ratio (see Camb. Trans., vol. xiv. p. 304).

May 16. C. CHREE.

A Comet observed from Sunrise to Noon.

A SHORT time ago I got the loan of an old number of *Harper's Monthly* (March 1889), good reading matter being very acceptable, however old, in this outlandish place, in which I read an article, on the origin of celestial species, by J. Norman Lockyer, F.R.S., Cor. Inst. France, that set me thinking of what I observed of the great comet of 1882, when it made its tremendous plunge round the sun, on September 18. At that time I was master of a small vessel, trading in the Society Islands; and on the day mentioned—in latitude 16° 25′ S., longitude 151° 57′ W. of Greenwich, a position about midway between the two islands Bolabola and Maupiti (the Maurua of Cook)—I saw, with the naked eye, the comet travel about 90° of the circle of the sun's disk, between sunrise and noon; but what made it most remarkable to us was that it should be possible for us, in a perfectly clear sky, to be able to watch it all, from sunrise to noon, with very little more distress to the eye than if in a clear night looking at a full moon.

Now, Sir, may it not be that this is partly a *proof* of the theory set forth by Norman Lockyer in the article above mentioned, viz. that comets are swarms of meteorites in collision, travelling through space, and that the outer invisible part of the swarm that formed this comet's nucleus had partially eclipsed the sun, like a veil over it? I am not aware if it was noticed by any competent astronomer or not, but the chances are that none had the splendid opportunity that we had to see the phenomena; so, Sir, knowing that men of science are always glad to get facts from observers in all parts of the world is my excuse for writing this to you, not knowing Mr. Lockyer's address. Thinking this, although late, may probably be of some interest to the scientific world, I leave you to do what you may think proper with it. WM. ELLACOTT.

Raiatea, January 30.

Graphic Daily Record of the Magnetic Declination or Variation of the Compass at Washington.

I BEG to call your attention to the enclosed reprint from the May Pilot Chart of curves of magnetic declination as recorded at the United States Naval Observatory at Washington. This reprint admits of reproduction more readily than the curves as shown on the Pilot Chart, being in black and white, and only reduced to two-fifths of true size (the reduction on the Pilot Chart itself being one-quarter). It will be interesting to this Office to elicit expressions of opinion relative to the advantages of the prompt publication of these curves. The experiment is to be tried for three months, but it is not likely to be continued longer unless certain decided advantages develop. It may be of sufficient interest to NATURE to republish these curves, and thus assist us in giving them wide publicity.

RICHARDSON CLOOER,
Washington, D.C., May 6. Hydrographer.

[We are unable to print the curves, but we may note that they are issued with the following explanation :—"These curves indicate graphically the true direction in which the magnetic needle at the Naval Observatory pointed during each instant from noon, March 29, to noon, April 30. The base-line shows a slight break at the end of each two hours, 75th meridian time, and the amount of westerly variation at any time is 4° *plus* the number of minutes represented by the height of the curve above the base line at that time, measured by the scale at the right or left margin of the diagram. The slight breaks in the curve itself occur when the chronograph sheets are changed. Although the daily change of variation at any one place, even in magnetic storms such as those that have occurred during the past month, is too small to be of any importance in practical navigation, yet it is thought that the prompt publication of these curves cannot fail to interest masters of vessels, as well as scientific men. The mean daily curve, which can be drawn by taking the average of many such curves, shows that there is a regular, though slight,

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[1] Cambridge Philosophical Society's Transactions, vol. xiv. pp. 467–483.